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ON THE COST-EFFECTIVENESS APPROACH  
TO MILITARY R & D: A CRITIQUE

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My paper will first record what I take to be the main limitations and hazards of the Cost-Effectiveness approach to clarifying choices in the military area in general, then develop some aspects of the management problem raised by these limitations and hazards, and finally review these problems with special reference to R & D decisions.

I

To begin with, I regard the CE technique as a most valuable tool for elucidating choices on military capabilities. However, the measure of its usefulness depends crucially on the sense of restraint with which the tool is applied and its product appreciated. From this point of view, I am concerned, not with the vulgar objections to the CE technique, but only with those that its advocates themselves admit. Since these limitations are well-known, I will state them in highly summary fashion.

First, as everybody knows, the CE approach has unlimited power when normative problems are answered by givens so that we face a pure

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and simple problem of maximization, and when all costs as well as benefits are known and can be quantified. This means that the technique is at its most useful when the objective or output is definitely fixed, that is, when there is only one dependent variable, and the sole task is to minimize the costs which are readily and accurately measured. In these cases, alternative means to achieving the objective differ only in this key variable, and choosing the cheapest means in this sense is the only problem.

On the other hand, it is generally agreed that the CE approach is less useful in clarifying choices when the employment of different means leads to appreciably different outputs. Its usefulness is the more restricted, the more incommensurable the outputs and the more appreciable and unmeasurable the social costs other than those quantifiable in terms of money; that is, the usefulness of the technique is the more limited, the less the problem is capable of uniform quantification. This limitation is really obvious since rational decision-making requires us to maximize the value of all benefits minus the value of all costs or disadvantages. I stress that the utility of the technique depends very importantly on the completeness with which costs and benefits are analyzed. I stress particularly other costs than money costs; for these can be of great variety and, it seems to me, they are easily lost sight of. They may be political, as when a particular choice causes great inconvenience to an ally; or when choice engenders a sharp decline in the morale of a military service. Surely, service morale is an asset and its deflation is, as such, a disadvantage. We might want to accept this disadvantage if the net benefits of a choice promise to be very substantial, but we should hardly ignore or neglect it. To the extent that costs and benefits cannot be measured with accuracy, and to the extent that the problem is one of deciding, in an inherently subjective manner, between different sets of costs and benefits, problems of choice are insusceptible to rigorous economic analysis.<sup>1</sup>

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<sup>1</sup>Cf. James R. Schlesinger, "Quantitative Analysis and National Security," World Politics, Vol. 15, p. 305.

A second great limitation of the CE approach results from imperfect information. In the military area, various incalculable uncertainties must be faced often. Costs may be uncertain, technology may be uncertain, the properties of military-conflict situations may be uncertain, and the reactions and capabilities of potential enemy nations are apt to be uncertain. This last uncertainty is of particular import; it is imperative that military choices be examined within a framework of interaction. An opponent's response to our choices may, after all, curtail or altogether nullify the advantages we seek. Nor is it enough to recognize the conflict aspects of this problem of interaction.<sup>1</sup> The possibilities of tacit or formal cooperation, that is, of non-zero-sum types of situations, may be equally significant.

I would, somewhat recklessly, add a third potential limitation of the CE approach, and that is the salience it inevitably attributes to the criterion of purely monetary costs. Of course, I realize that, in a world of scarce resources, this must be an important yardstick. But, making the outrageous assumption for the moment, that other costs or benefits lend themselves as readily to quantification as estimated money costs do, should we not consider whether, in societies becoming ever more affluent, monetary costs, though important, should not be expected to decline in importance relatively to other values, positive or negative. For example, would the minimization of military conflict, or of loss of human life in the event of conflict, not gain importance in relation to monetary sacrifice? This consideration should certainly make a claim on our attention whenever the monetary sacrifice involved in choices is, absolutely speaking, rather small. Thus, if we wanted to apply the CE technique to assessing the worth of the cost-effectiveness approach itself, its financial costs would probably be regarded as relatively trivial, and we would look toward more significant criteria of the disadvantages

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<sup>1</sup>This exclusiveness is suggested in E. S. Quade, "Methods and Procedures," in E. S. Quade (ed.), Analysis for Military Decisions, The RAND Corporation, R-387-PR, November 1964, p. 152.

of cost-effectiveness studies. Now, other criteria do not, unhappily, permit quantification nearly as much as money costs do. But, to the extent this is the case, and accounts for our preference to focus on money costs, are we then not saying that the value of this focus on monetary costs is derived from the convenient fact that they are capable of easy measurement?

There is general agreement that the two major limitations I stated first greatly restrict the usefulness of the CE approach in making high-level decisions on military matters, for such decisions do involve choice-of-objective problems and bristle with intangibles and uncertainties. But it is to complex problems of this kind that the CE technique is in fact applied, and should be applied. To do so is, if rightly done, entirely proper and unquestionably useful. Not only are differences in money costs usually important, to proceed rationally we must obviously also regard all advantages of a policy as a return, and all disadvantages as a cost, and define the best policy as the one which offers the largest margin of return over costs.

## II

However, it is precisely at this point that we encounter the problems of management. One problem is that the value of CE analysis is sensitive to the resources and time given to it, and that hasty analyses may do more harm than good. A critical factor along this line may be the tendency for the insistence of the budgetary cycle to deny sufficient time.

A second problem is that, partly resulting from time pressure, CE studies are fragmentary, that all costs and benefits do not receive due attention, and that money costs claim undue emphasis. To put it more crudely, we may have too many cost studies and not enough cost-benefit studies; analyses overemphasizing money costs may just pretend to be CE studies.

This leads me to the related third problem that the CE approach

be given no more influence on decisions than, in view of the inevitable or practical limitations of CE studies, it can legitimately claim. The CE technique may be a scientific technique; yet its application is an art; that is to say, an activity heavily dependent on imagination and judgment. There is nothing wrong with CE as a tool; but a great deal could be wrong with its exploitation if it is not governed by an inventive imagination, good judgment, and a measure of humility. Let me spell out briefly some additional dangers arising in its exploitation; and it is clear to me that nearly all these dangers have been discovered and acknowledged by proponents of the technique.

The central problem is that CE studies must count for no more, and no less, than their due. They got less than their due before Mr. McNamara became Secretary of Defense. The question is whether there is not now a tendency in the Department of Defense for CE studies, suffering from the lack of balance I have discussed, to receive more than their due. If it were true, this would be a serious matter, and especially so in the case of important, very high-level decisions regarding which, it is generally agreed, the CE technique can make only a very limited contribution, even though a valuable one, within these narrow limits. I do not know that there is now a tendency in the DOD for CE studies to be accorded excessive attention and weight. I suspect that this is so. I suspect rather than know because what evidence I have is very little and rather soft.

The evidence is of two kinds. First, a considerable number of people who have been close to the decision-making process, and whose judgment I respect, have told me that the tendency prevails. Second, I read with care Mr. McNamara's 1963 testimony on why he preferred a conventionally-fueled to a nuclear-fueled aircraft carrier. In his very lengthy testimony, Mr. McNamara came back again and again to the difference in money costs but, though several Senators pressed him with intelligent and pertinent questions, he never explained why the advantages of the nuclear carrier were not worth the difference in these costs. He contented himself with stating flatly that he

did not think they were, while citing eagerly and at length some dubious analogies of why he personally was better off buying a medium- rather than high-price automobile, or why a farmer, having to transport produce to the market from time to time, might be better off with a cheaper and slower than with a speedier and more expensive truck. The trouble with these analogies is that they explain the CE principle -- especially, a stripped-down economy version of it -- but that they do not explain at all the superiority as a buy of a conventional over a nuclear carrier. Obviously, Mr. McNamara has far better information on the factors affecting his choice of automobile than he could possibly have about the future utility of different aircraft carriers in different contingencies whose probabilities are unpredictable. He also could easily make some simple assumption about the hypothetical farmer's transportation problem, while similar assumptions about the future missions of aircraft carriers are more difficult to make. I could not help getting the strong impression that the money-cost difference and the stripped-down CE model were foremost in the Secretary's mind, and that the very complicated guesswork on possible demands on aircraft carriers some years hence was not.

This leads me to the proposition that the structure of attention paid by decision-makers to complex problems of choice should accord no more salience to CE studies than they can properly claim. This precept is more easily expressed than implemented. Implementation requires that the total problem be subjected to orderly conceptualization, that the intangibles as well as the quantitative factors be properly analyzed, and that an attribution of relative priorities guide the decision-maker in how to bring various parts of the analysis to bear on the problem of choice. This last condition is very important in a bureaucracy in which decisions are prepared at various layers on a decentralized basis. Even if these rules are observed, the danger remains that the top-level decision-maker may be excessively attracted by the neatness of quantitative analysis and conclusions, and that he may neglect those parts of the analysis that

are iffy, perhaps obscure, messy, and certainly hard to evaluate. Even a practitioner like Mr. Hitch concedes this "potential hazard" to which CE studies may give rise.<sup>1</sup> One antidote is no doubt the development of effective models for qualitative analysis. But they will be very useful only if the top decision-maker does not nurse a vulgar skepticism regarding non-economic models, and if there are personnel with an adequate range of skills to design and work these models.

Indeed, excessive reliance on economists and other experts on quantitative analysis is another condition that may cause the presentation of problems of choice to be slanted in favor of CE analysis that overemphasizes money costs. It is not only that the CE experts do not necessarily command expertise on essentially military, political, and psychological problems, it is also that they tend to have acquired perceptual propensities more suited to some problems than to others. In this respect, it is interesting to note the praise lavished on these propensities by Charles Hitch. He approves of "...economic choice as a way of looking at problems...;"<sup>2</sup> he lauds the "quantified common sense" of the systems analyst;<sup>3</sup> and he remarks that systems analysis "...provides the checks and balances so essential to minimizing parochial viewpoints..."<sup>4</sup> Hitch is quite right in extolling these virtues. Yet this style of thought is virtuous only within the proper context and, when pressed beyond, virtue could turn into vice. "Quantified common sense" may give short shrift to the analysis of intangibles, and systems analysts could develop a parochialism of their own unless there are checks and balances -- and I do not mean service biases -- to supplement their intellectual habits. Such

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<sup>1</sup>Cf. Quade (ed.), Analysis for Military Decisions, pp. 326-327; see also Quade, "Methods and Procedures," ibid., p. 170; Schlesinger, op. cit., p. 300.

<sup>2</sup>Charles J. Hitch, Decision-Making for Defense, Berkeley, 1965, p. 53.

<sup>3</sup>Ibid., p. 55.

<sup>4</sup>Ibid., p. 57.

checks and balances demand sophisticated personnel with a different range of expertise.

The proponents of CE studies admit that this management problem exists, although they express it perhaps more vaguely than I have done. Mr. Hitch avers that the dangers are known and hence controlled. But Schlesinger has observed that "...a ritualistic recitation of the dangers of excessive quantification characteristically precedes the attempt to push quantitative analysis too far."<sup>1</sup> Surely, the mere argument by the practitioners that the problem is under control will not do. The argument may be truthful or not. One must, therefore, insist that whether or not the dangers of CE have been avoided and are avoided in the DOD is an empirical question that cannot be settled by argument. To convince me that they are avoided would require a thorough study of past problem-solving by impartial researchers. This could be done by a number of properly selected and properly conducted postmortems. I do not in fact know whether or not such postmortems have been undertaken. If they have not, the determination with which the CE technique is applied to problems of great national importance makes it imperative that such a study be undertaken. To do so is in fact in the spirit of systems analysis.

There is another way of studying this management problem empirically. Psychologists could explore the relevant conditions that tend to slant the perception and prejudice the attention of policy-makers and their assistants when confronted with highly complex problems of choice. I do not mean that decision-makers should be psychoanalyzed. What I have in mind is that experimental psychologists have already elucidated many of the conditions that affect perception and attention, and that it should be possible for them to explore these problems within the context of our interest. How does the presentation of policy analysis, some quantitative and some qualitative, some based on solid information, and some on surmise, impinge on perception and attention? If such empirical efforts produced more knowledge than we now have, we would be in a better position to engineer improvements in the art of exploiting CE

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<sup>1</sup> Schlesinger, op. cit., p. 306.

analysis. If this advocacy on fact-finding research strikes anyone as being superfluous, let me remind him of one thing we do know about human behavior, namely, that cognizance of a problem does not lead automatically to its solution.

### III

Now I will turn briefly to a review of our management problem with particular reference to choices in military R & D. The limitations on the usefulness of CE studies, and the management risks to which such studies give rise, are much greater when the CE technique is applied to R & D choices than to military choices in general. Clearly, to decide on preferring a conventional aircraft carrier to a nuclear-powered carrier in 1963 was far easier a problem (though not apparently an easy problem) than to decide earlier on that a nuclear carrier should be developed.

In the case of R & D choices, the uncertainties tend to be greater. As experience shows abundantly, financial costs are hard to estimate; technological advance is difficult to predict, and the benefits are hard to evaluate. After all, R & D outputs will affect military capabilities only with a considerable time lag and -- during this time -- the relevant military, technological, and political environment may undergo substantial changes that impinge on the value of a weapon system, or of a strategy for which it is designed. Above all, the capabilities of potential enemies may change significantly, in part perhaps as a result of their reaction to our R & D choices. Even the actual use of evolving weapons is hard to predict; at least, history provides numerous examples of new weapons finding uses quite different from what they were originally intended for.

The impingement of these conditions that are hard to cope with will, of course, vary a great deal. Their impingement will tend to be the greater, the more R & D projects involve true innovation rather than marginal improvement. This proposition follows from the fact that the more innovation is involved, the greater will be the

uncertainties. Their impingement will also tend to be the greater, the earlier the phase of R & D, again because at this stage the uncertainties are greater.

It is true that the CE technique is not applied at present to the most initial phases of R & D proposals -- Research and Exploratory Development -- as long as the estimated costs fall short of a certain amount. The questions are whether the cut-off is well-chosen and whether a fixed cut-off is in principle sound.

At any rate, if the enthusiastic practitioner of CE is apt to exaggerate the assistance his technique can give to the policy-maker, this is especially dangerous in the case of R & D choices. The worst danger would be if the insusceptibility to CE studies of highly innovative projects led to a bias favoring projects that are more susceptible. In the case of R & D proposals involving a high degree of innovation, it would seem relatively more important to seek advice from people adept at making conjectures about the future military and political environment, and on the military needs which changes in this environment may generate. Only by doing so can we reduce consumer ignorance at the higher levels of decision-making where the insertion of judgment is crucial.

By way of conclusion I will repeat my plea for empirical studies of the values and dangers of CE guidance. Surely, to find out more about how to manage CE is specially important with reference to military R & D. Indeed, the case for CE would gain in strength if we learned more about the art of its application. If we did so, and particularly if we insisted on comprehensive CE studies, I would certainly conclude: let us have more of them.